The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-253-RC1, 2019

The Cryosphere Discussions
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Interactive comment on "A model for French-press experiments of dry snow compaction" *by* Colin R. Meyer et al.

Anonymous Referee #1

Received and published: 8 December 2019

This paper outlines a theoretical model to describe previous snow compaction experiments in a 'french-press'-style setup. The model describes unidirectional compaction of a plastic two-phase mixture, and is based on previous modelling derived for mixtures of deformable solids in a liquid suspension. The authors demonstrate that the model can give reasonable agreement with the previous experimental measurements, suggesting that this way of describing and snow-air mixture is plausible, and providing a possible means to calibrate material properties (compressive strength and permeability).

The paper is well written, clear and informative, and I think it should be published in this journal. I have only a few comments/questions that the authors could consider:

1) It would be interesting to report (and plot) how much differential compaction there

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Discussion paper

is in the samples for the experimental comparison in figure 4. The values quoted for gamma are fairly small, which suggests that there is quite a significant gradient of porosity (or effective stress/pressure) - i.e. the experiments were in a regime more like fig.3(b) than fig.3(a). Presumably Wang and Baker (2013) had some sense of whether they thought they were compressing in such a manner that the sample remained uniform in depth or not (?), and it would be interesting to see what the theory predicts. What would the large-gamma limit curves look like for the different experiments reported in figure 4, and how much above them is the load that was actually measured?

2) What is pore-scale Reynolds number of the air flow in the experiments? Is it always sufficiently low that Darcy's law can be reasonably assumed? One might also consider air compressibility - is it always reasonable to assume the density is constant in equation (3)?

3) At the end of the paper, the concept of viscous compaction is briefly introduced. This is interesting, and raises the question of how one would tell whether the snow compressed by Wang and Baker was behaving plastically or viscously. The authors state that the viscous theory can't capture the evacuation of air, but this isn't really fair - it would be perfectly possible to derive the same model (i.e. a two-phase model capturing the motion of the air) used by the authors but with a viscous closure law for the effective pressure in place of the plastic one in (16). (i.e. using (33), with N taking the place of P_e). I can't immediately see why this would not be able to capture the behaviour observed in the experiments. It seems important that there is some discussion of this.

Some typos:

Equation (3) and (4) have 'm_s' in them, but 'm' is reported in the following sentence. Equation (18) should have a 'z' not an 'x'. Superfluous comma after 'Although' on line 9 of page 12.

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